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THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. VI. ANN ARBOR, SEPTEMBER, 1889. No. 5.

ORIGINAL ARTICLES.

CLOUD FORMATION.

BY PROF. H. A. HAZEN.

Cloud and fog are precisely the same except that fog is formed near the earth and usually has more dust in it. Careful examination has shown that minute globules of water $\frac{1}{1000}$ to $\frac{1}{7000}$ in. in diameter constitute fog. The reason for their suspension has been supposed by some the same as for the support of dust particles, by others it is thought that some form of electrical action is concerned in their suspension. Some experiments have shown the velocity of fall of mist in a receiver from 300 to 400 ft. per hour. It is very evident that there must be some force acting to prevent a rapid fall of the fog particles or else there is a continued renewal of them.

While other sciences demand that their theories receive demonstration by experimental research or otherwise, yet meteorology is unfortunate in that its theories, even where experiment would seem possible, are accepted without question by many of those who put them forth. Other sciences build up theories little by little as facts accumulate, and these are well grounded upon positive knowledge, but it has been declared that meteorology has yet to begin such work. It is easy to see

that the processes of nature in cloud and storm generation are exceedingly complex and that experimental research will be attended by great obstacles, but all the more, on these accounts, ought we to doubt our theories and invariably put them to a practical test.

It may be said that we cannot exactly reproduce nature's processes in our laboratories, this may be granted, but can we not formulate our theories and put them to a practical test? if we cannot, our theories must go. It would be far preferable, to question nature herself, to go to the spot where she acts and try to ferret out her mysteries, but this is impracticable just at present. We must take great care in interpreting our puny efforts, but, when it comes to a comparison between vague theories and experimental research, it seems to me the latter must carry off the palm.

Here is a single example of a theory. Let us assume that heat and moisture are the only factors concerned in storm generation. It is not difficult to reason that the sun heats up a portion of the earth's surface, that this sets up ascending currents, which rise to the upper atmosphere, are cooled by expansion and by the cooler air above, then cloud is formed by the condensation of the moisture. If the process goes on a sufficient length of time, the velocity of the uprushing current is enhanced by the less rapid cooling of the air in it, because of the liberation of latent heat attendant upon the condensation, and finally rain begins to fall. It seems to me this theory as a whole is admirably put together and yet probably it is a mass of false reasoning from beginning to end.

Why should we assume that heat and moisture are the only elements that enter storm generation? We have abundant evidence before our eyes that electricity in a storm vastly exceeds that in a clear sky and its action is very different. Notwithstanding, it has been regarded as a result of storm conditions and not in any manner producing them. This certainly is a violent assumption. Let us ask further. Does the sun act as if through a burning glass and heat up the earth? This is the theory of some who claim that our high areas

of pressure constitute a convex lens and concentrate the sun's rays, but, unfortunately for this theory, our cool weather comes with this condition. Is it not plain that the sun heats up an area about 1,000 miles in diameter, and the temperature very gradually falls off on all sides from this? But suppose the uprushing currents have started and cloud formation has begun, it is evident that the clouds at once act as a curtain and shut off the sun's heat. Balloon observations have shown conclusively that the air temperature is nearly uniform from the earth's surface to and through the lower portion of the cloud stratum. It is evident that this process, even if begun, would be quickly brought to rest.

We have, however, a still stronger proof of this reasoning, if one were needed. Let us grant that there is an ascending current in the center of a storm, which is a convenient hypothesis but without much doubt untrue. Is that the region of highest temperature? By no means, but the heat is concentrated 400 or 500 miles to the south and southeast. Is that the region of rain fall? Certainly not, but to the south and southeast as before. This whole theory originated years and years before we had weather charts, and these have effectually disposed of it. Does the condensation of moisture liberate latent heat? If it did, would not the result be reëvaporation of the moisture? Must not this supposed cooling be rather a heating of the surrounding air which would unsaturate it at once? Is the effect of an ascending current one whit different from that which would be produced by mixing two bodies of air of different temperature? In this latter case it is conceded that no effect would result. This example will suffice to show that it would be well to ascertain from the clouds themselves a few facts before we press our theories.

Is it possible to demonstrate the amount of cooling produced in air by a certain degree of exhaustion? The difficulties may be summed up as follows:

1st. Although air is not a conductor of heat, yet, when we cool it by diminishing its pressure, heat very rapidly passes to the thermometer from the sides of the vessel, and it has

been found very difficult to cool the sides at the same time with the air inside.

2nd. In saturated air there is a tendency to deposit moisture on the thermometer bulb and complicate the result.

3rd. The most serious difficulty, however, is in the sluggishness of our thermometers. I used one which would fall 1° in two or three seconds. It had a bulb only .06″ in diameter and was exceedlngly rapid in its fluctuations; by carrying on the exhaustion or compression very slowly, it was possible to obtain a fair idea of the amount of cooling or heating.

I found the amount of cooling on exhausting 1″, 5″ and 10″ only about one-fourth of what theory would indicate, and the amount of heating by compression also about one-fourth of theory. This furnishes a pretty good proof that the results were not far from the truth, for the reason, that the thermometer acts much more rapidly with rising temperature than with falling. The amount of fall in temperature for an exhaustion of 1″ was less than .7°, that is, if a mass of air were suddenly transported upward 1,000 feet, the total cooling due to expansion would be less than .7° which is practically inappreciable. The cooling due to the height above the earth would be four or five times as great. These results are only approximate, but I am confident they are somewhat above the actual amount.

One rather interesting fact was noted. When the air was compressed the sudden release from pressure brought the thermometer back to its original reading almost invariably, but when the air was exhausted and readmitted, the final reading was higher than the initial. This might be thought as due, in part at least, to the slower action of the thermometer with falling temperature. But other experiments showed that by conducting the process at a limited speed the difference from this cause was slight. Probably there was an actual enhanced rise in temperature owing possibly to friction of the particles against each other and the vessel with the readmission of air. It was thought that the outside air which was forced in and continuously departed more and more from the temperature of the air in the vessel, might have a slight effect upon the result. To

test this, the mouth of the tube for compression or exhaustion was placed 1st, near the thermometer; 2nd, as far as possible from it. The result showed a very slight cooling, less than .05 of the whole amount, on compression, and about the same heating on readmission of the air.

The results with dry and moist air were almost exactly the same, though in the latter case the cooling on exhaustion and heating on compression was slightly less. The results were complicated in moist air by the deposition of moisture on the sides of the vessel and on the thermometer. All the experiments were conducted, both with dust free air, and with air surcharged with smoke from burning cotton, with no appreciable difference in the results.

Experiments were also made to determine the effect of dust or smoke on cloud formation, and here there was no uncertain sound, though in the results just described everything must be regarded as only approximate. The best method of saturating air was found to be by passing it through a bottle containing sponge saturated with water. If water itself or broken pumice stone were used, the air passed through in little bubbles, but with sponge there were no bubbles. A few minutes of time only were needed, with the sponge bottle, to bring the readings of wet and dry thermometer together, while in the previous experiments, described in "Science" for June 21, it was found impossible to saturate the air. The best way of rigging the wet and dry thermometers is to put them in a small bottle and connect this by a tube with the large receiver, this avoids a complication of the experiments, especially in dry air, by the presence of the wet bulb.

It was quickly found that a hazy appearance could be produced in very dusty air, clear of course before the experiment, when it was far from moist. In practically dry air this haze could be produced and was possibly due to a sudden bombardment of the dust particles or aggregation of them on the release of pressure. The haze was exceedingly evanescent. Three kinds of aggregations were noted in moist air. The first I will call "haze," the second "mist" and the third "cloud." These

forms were perfectly distinct and could be produced at will. The first was noted in moist or dry air when dust or smoke was present. It had a peculiar dry appearance, precisely similar to haze in the sky, it did not appear at all like fog or cloud. This would seem a satisfactory explanation of a peculiar haze that was entered into in a balloon voyage on June 26, 1886, near Providence, R. I. At 2,500 feet, the air had a relative humidity of about 70 per cent. and yet the haze was so dense that we could not see more than ten or twenty feet. It was just like a very dense, dry fog. It will be understood that this haze was not produced with the same amount of compression in dry as in moist air. While a compression of 10 mm. and sudden expansion gave the haze in moist air, a compression of 400 to 500 millimeters (16 to 20 inches) was needed in dry air. Many of the experiments were repeated with exhaustion by the air pump, but they were not quite as satisfactory as the others.

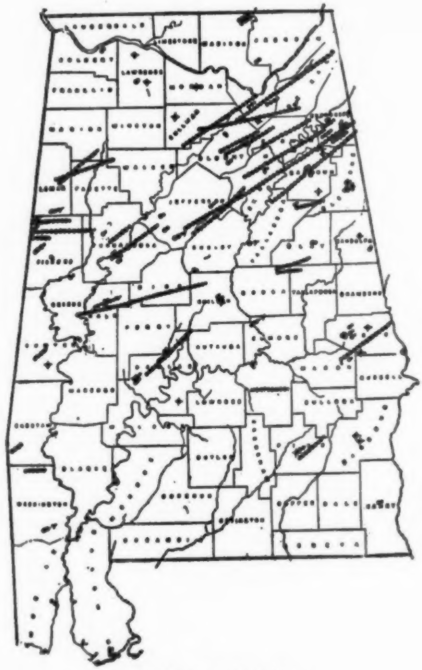
It was not a very difficult matter to draw out all the smoke particles in the air, in fact, it was necessary to continually renew them, especially in dry air. An interesting phenomenon was seen on introducing the smoke (which was best done by lighting a piece of cotton twine and passing the glowing coal into the receiver), the smoke formed peculiar surfaces, sometimes rounded very similar to vortex rings, but without a rotation. The fall in dry air was much more rapid than in moist, and of the rings much more so than of the uniform haze, which gradually disappeared without reaching the bottom. The rapidity of fall noted was 200 feet per hour, and from that to practically nothing.

The second form, "mist," was noted in moist, dust free air either by compression or exhaustion. It presented a beautiful appearance, the whole receiver was filled with fine, distinct, rounded particles which gradually settled to the bottom. I have had no means at command for measuring the diameter, but they were so small that they could only be seen by allowing them to pass between the eye and a light. They certainly were formed in absolutely dust free air and seemed to be aggregations of moisture produced possibly by a gradual cooling of the air. The

best way to form this mist was either by exhausting with the air pump or by compressing the air and then allowing it to escape gradually.

The last form, "cloud," was in many respects the most interesting of all. On compressing dust free air 12 or more inches and either blowing out the large stopper or else forcibly removing it, the whole receiver was filled with a beautiful damp cloud or fog. There were hardly any mist particles visible, though these may have been hidden by the fog, but probably not. The fog had a very slow settling movement, giving a perfectly level top, and entirely different from the previous dust haze which disappeared almost at once throughout the receiver. The velocity of fall was about 10 feet per hour. This fog was certainly formed in absolutely dust free air, and was caused by the very sudden expansion of the air. If the compression and expansion were produced very suddenly the effect was not nearly as marked as when the compression was accomplished gradually, allowing the heat to pass out from the receiver. This showed that cooling was necessary to produce the phenomenon at its best. On continuing the experiments in compression with this air, in which the cloud had formed, it was found that the cloud particles, though rendered invisible by the compression, acted very much as the previous smoke particles had acted, that is, a partial cloud could be formed by compressing very slightly and then releasing the pressure. It would seem that the union of the vapor molecules was not entirely broken up by the compression and increased heat, but the ultimate water particles acted like the previous smoke. These water particles could easily be removed from the receiver as the smoke had been before. I have given the results of over a thousand experiments made under as varying conditions as I could possibly desire. It seems to me the field for experiment, in this line, is boundless, and while I, by no means, consider the questions here mentioned as settled, with the exception of the production of cloud in dust free air, yet it seems to me wise to give out these results, hoping that others will take up the research.

July 5th, 1889.



TORNADOES IN ALABAMA.

STATE TORNADO CHARTS.—ALABAMA.

BY LIEUT. JNO. P. FINLEY, SIGNAL SERVICE, U. S. A.

TABLE I.—*Tornadoes in Alabama.*

Period of observation, 67 years, 1822-1888.
Total number of storms, 112.
Year of greatest frequency, 1884,—19 storms.
Average yearly frequency,—1.6 storms.
Year in past (10) ten years no report of storms,—none.
Month of greatest frequency, March,—28 storms.
Day of greatest frequency, January 11th,—7 storms.
Hours of greatest frequency, 6 to 7 P. M. and 7 to 8 P. M.
Months without storms, July, August, September and October.
Prevailing direction of storm movement, NE.
Region of maximum storm frequency, north central portion.

TABLE II.—*A Chronological Table, showing the location, date and time of occurrence, and general character of formation and movement of Tornadoes in the State of Alabama for a period of 67 years, from 1822 to 1888.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Morgan	April 16.	1822	5 p. m.	NE.	Funnel.	2,640.
Chilton		1823		E 10° N.		2,640.
Morgan	April 6.	1823	9 p. m.	NE.	Funnel.	3,960.
Tuscaloosa	April 25.	1829				
Calhoun	May 1.	1830				
Morgan	June 16.	1834	4:30 p. m.	NE.	Funnel.	1.237.
Blount		1840		E 20° N.		
Etowah		1840		E 20° N.		
Blount	March 10.	1840	6 p. m.	E.	Funnel.	2,640.
Jefferson	March 16.	1840	About 6 p. m.	NE.		1,320.
Mobile	March 24.	1840	7 p. m.	S 80° E.		165 to 660.
Tuscaloosa	March 4.	1842	6 a. m.	NE.		
Tuscaloosa		1843		E 45° N.	Inverted Cone.	600.
Lee	March 7.	1854	1 p. m.	NE.		900.
Pickens	March 12.	1855		SE.		
Cherokee	May 24.	1857		NE.		
Lee		1858		NE.		1.320.
Cleburne	November 30.	1861	10 p. m.	NE.		600 to 900.
Cleburne	March 4.	1863	11 p. m.	NE.		600 to 900.
Lee	December 25.	1864	Midnight.	NE		
Cherokee		1866		E 40° N.		
Cleburne	April 16.	1866	11 p. m.	NE.		600 to 900.
Talladega	May 6.	1866	8 p m.	E 10° N.		1,320.
Calhoun	February 15.	1867		E 10° N.		
Tuscaloosa	April 29.	1867	10 a. m.	E 20° N.	Funnel.	
Cleburne	May 4.	1867	Midnight.	NE		600 to 900.
Cleburne	May 26	1867	8 p. m.	ENE.	Funnel.	600 to 900.
Cleburne	February 12.	1868	3 p m.	NE.		600 to 900.
Tuscaloosa	May 8.	1868				
Clay	January 29.	1869	8:30 a. m.	E.	Funnel.	1,320.
Talladega	April —.	1869		NE.		
Pickens	May 6.	1869	6 p. m.	E.		450.
Calhoun	January —.	1870	8 a. m.			
Marshall	April 23.	1870		NE.		
Calhoun	December 24.	1870			Funnel.	
Cleburne	November 16.	1873	12 p. m.	NE.		600 to 900.
Jackson and Calhoun		1874				

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Hale	November 22	1874	Afternoon	SE.		
Colbert	"	1874	6 p. m.	NE.	Funnel.	900.
Shelby	"	1874	Midnight.	E 10° S.		300 to 1,320.
Dallas	November 27.	1874			Funnel.	
Cherokee		1875		E 30° N.		1,320.
Lamar	February 24.	1875	6:30 p. m.	NE.		450.
Lee	March 20.	1875	2 p. m.	NE	Funnel.	300 to 900.
Pike	"	1875				
Coosa and Tallapoosa	May 1.	1875	About 10 a. m	E.		1,320.
De Kalb	April 23.	1876		NNE.		
Etowah	December 25.	1876		E 20° N.		
Hale	April 23.	1878	5 p. m.	NE.		2,640.
Chilton	February —.	1879				360 to 600.
Chilton	February 13.	1880		ENE.		1,320.
Barbour	March 15.	1880	Evening.			
Pike	March 18.	1880	"	NE.		
Jackson	April 25.	1880	Afternoon.	NE.	Funnel.	1,000 to 3,500.
Blount	December —.	1880		SE.		
Cherokee		1881		ENE.		
Tuscaloosa	February 18.	1881	Afternoon.	NE.	Funnel.	Narrow.
Perry	February 26.	1881	Midnight.	NE.		300.
Randolph	March 22	1881	1 a. m.	NE.	Funnel.	
Sumter	March 23.	1881	5 p. m.	NE.	"	80 to 120.
Madison	February 28	1882	11:45 a. m.	NE.	"	900.
Barbour	March 27.	1882	Night.			5,280.
Dallas	"	1882	"			
Henry.	"	1882	9:30 p. m.	E.		
Lee	"	1882	Night.			
Washington	"	1882	2:30 p. m.	E.		1,320.
Etowah	April 2.	1882		NE.		
Choctaw	April 22.	1882	4 p. m.	NE.		1,200.
Jefferson	January 16.	1883		NE.	Funnel.	
Blount	April 2.	1883		SE.		
Jefferson	April 22.	1883		NE.		600.
Cherokee	"	1883	10:30 p. m.			900.
Cherokee	April 23.	1883	4:30 p. m.	NE.		1,320.
Fayette	January 11.	1884	6 p. m.	E.	Funnel.	1,200.
Pickens	February 19.	1884	12 m.	NE.		
Pickens	"	1884	11 a. m.	NE.		

TABLE II.—*Concluded.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Talladega and Calhoun	February 19.	1884	2 p. m.	NE.	Balloon.	1,320 to 3,960.
Marshall	"	1884	9 p. m.			1,320.
Jefferson and St. Clair	"	1884	1:20 p. m.	NE.	Funnel.	600 to 2,640.
Cherokee	March 6.	1884	4 p. m.			450.
Tuscaloosa	March 11.	1884	10:30 p. m.	NE.		1,320.
Pickens	"	1884	7 p. m.	NE.	Funnel.	300 to 900.
Marshall	"	1884	7:30 p. m.			900 to 1,200.
Greene	"	1884	8 p. m.	NE.	Funnel.	300.
Jefferson	March 25.	1884	2 p. m.	NE.	"	300.
Cherokee	"	1884	8 p. m.	NE.	"	
Lawrence and Jackson	April 1.	1884	6 p. m.	NE.	"	900 to 1,320.
St. Clair	"	1884	Midnight.	NE.	"	
Blount and De Kalb	"	1884		NE.		
Lawrence	April 12.	1884	2:30 p. m.			
Lawrence	"	1884	"			
Lee	December 12.	1884	Night.	NE.		100.
Randolph	January 11.	1885				
Macon	"	1885	11 p. m.	E 20° N.		2,640.
Coosa and Clay	"	1885	9 p. m.	E 10° N.	Funnel.	1,320 to 2,640.
Lamar, Fayette and Walker	"	1885	5 p. m.	E 15° N.		1,320 to 2,640.
Greene, Hale, Bibb and Chilton	"	1885	6:20 p. m.	E 30° N.	Funnel.	900 to 1,320.
Cullman, Blount, Marshall and De Kalb	"	1885	7 p. m.	E 20° N.		600 to 3,960.
Coosa	February 20.	1885	5:30 p. m.	NE.	Funnel.	250.
Marshall	March 28.	1885	Afternoon.			300.
Madison	May 6.	1885	6:30 p. m.	N 45° E.	Funnel.	Narrow.
Sumter	November 6.	1885	8 p. m.	NE.		1,320.
Lamar	"	1885	10 p. m.	NE.		300 to 2,700.
Dallas	"	1885	3:30 p. m	NE.	Funnel.	300 to 2,640.
Hale	April 25.	1886	Afternoon.	NE.		
Elmone	March 29.	1886	Morning.	NE.	Funnel.	450.
Washington	January 13.	1887	"	NE.	"	1,320.
Jefferson	April 18	1887	3 a. m.	NE.	"	Narrow.
Lamar	April 22.	1887	6 p. m.	NE.	"	900.
Pike	June 26.	1888	Afternoon.	Easterly.	"	Narrow.
Talladega	"	1888	"	"	"	"

TABLE III.—*Relative frequency of Tornadoes by months and days, for Alabama.*

The index figures to the right and above the dates show how many times tornadoes occurred on that day of the month.

Month.	Day of Month.	No. of Days.	Total No. of Tornadoes per month.
January	$(11)^7$, 13, 16, 29 and (—)	5	11
February	12, 13, 15, 18, $(19)^5$, 20, 24, 26, 28 and (—)	10	14
March	$(4)^2$, 6, 7, 10, $(11)^4$, 12, 15, 16, 18, $(20)^2$, 22, 23, 24, $(25)^2$, $(27)^5$, 28, 29 and (—).	18	28
April	$(1)^3$, $(2)^2$, 6, $(12)^2$, $(16)^2$, 18, $(22)^4$, $(23)^4$, $(25)^3$, 29 and (—)	11	24
May	$(1)^2$, 4, $(6)^3$, 8, 24 and 26	6	9
June	16, $(26)^2$	2	3
November	$(6)^3$, 16, $(22)^3$, 27 and 30	5	9
December	12, 24, $(25)^2$ and (—)	4	5
(—) Blank	$(—)^9$	1	9
Total		62	112

NOTE.—The blank (—) signifies date missing.

TORNADOES IN OHIO.

STATE TORNADO CHARTS.—OHIO.

BY LIEUT. JNO. P. FINLEY, SIGNAL SERVICE, U. S. A.

TABLE I.—*Tornadoes in Ohio.*

Period of observation, 85 years, 1804–1888.
Total number of storms, 133.
Year of greatest frequency, 1886,—33 storms.
Year in past ten (10) years no report of storms, 1882.
Average yearly frequency, 5.32.
Month of greatest frequency, May,—45 storms.
Day of greatest frequency, May 14th,—16 storms,
Hour of greatest frequency, 4 to 5 P. M.
Months without storms,—none.
Prevailing direction of storm movement, NE.
Region of maximum storm frequency, middle western, southwestern and extreme eastern portions.

TABLE II.—*A Chronological Table, showing the location, date and time of occurrence, and general character of formation and movement of Tornadoes in the State of Ohio for a period of 85 years, from 1804 to 1888.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Geauga	August.	1804				
Hamilton	May 28.	1809	2 p. m.			Very narrow.
Champaign	Summer.	1818	3 p. m.	Easterly.		1,320.
Jefferson	February 19.	1820	p. m.	NE.		
Miami	May 11.	1823	2 p. m.	NE.	Funnel.	1,500 to 5,280.
Miami	May 13.	1823	2:30 p. m.	NE.	"	3,200 to 5,280.
Cuyahoga	August 22.	1822	10 a. m.			
Ashtabula	June 19.	1823	9:30 p. m.	E.	Funnel.	1,320 to 1,650.
Licking		1830	Afternoon.	NE.	"	
Champaign	March 22.	1830		NE.		
Clark	April 11.	1833	Afternoon.	NE.		
Summit	October 20.	1837	3 a m.	E.		1,280.
Lucas	May 23.	1839	Afternoon.	NE.		
Jefferson	Autumn.	1841	Afternoon.		Funnel.	
Cuyahoga	February 4.	1842	4:30 p. m.	N 33° E.	"	1,650 to 4,372.
Miami	February 4.	1842	Afternoon.	NE.	"	
Knox	January 20.	1854	2 p. m.	N 56° E.	"	1,760.
Hamilton	February 14.	1854	p. m.	N 72° E.	"	200 to 600.
Hardin	June 2.	1875	p. m.	NE.	"	
Putnam	July 4.	1875	4:30 p. m.	NE.	"	660.
Clermont	August 1.	1875	3 p. m.	NE.		
Hardin	December 20.	1875				
Mahoning	August 15.	1876				
Mercer	June 21.	1877	Afternoon.	NE.		
Butler	June 28.	1877	Afternoon.	NE.		
Butler	July 5.	1877	4:20 p. m.	SE.		
Morrow	"	1877	Afternoon.			
Cuyahoga	August 31.	1877				
Erie	July 11.	1879	3 p. m.	SE.	Funnel.	
Logan	"	1879	Afternoon.			
Huron	November 14.	1879	11 a. m.	NE.		
Montgomery	January 9.	1880				
Hamilton	February 13.	1880		SE.		5,280.
Hamilton	February 18.	1880				
Lucas	March 4.	1880	11:15 p. m.	NE.	Funnel.	
Huron	May 21.	1880	5:30 p. m.	N 2° E.	"	
Huron	May 22.	1880	5:20 p. m.	NNE.	"	240 to 500.

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Hamilton	June 14.	1880	8 p. m.	NE.	Funnel.	....
Butler	July 2.	1880	4 p. m.	NE.	Hour-glass.	600 to 1,000.
Franklin	May 14.	1881	Afternoon.	NE.	....	....
Huron	June 16.	1881	Afternoon.	NE.	....	....
Seneca	July 7.	1881	Evening.	NE.	Funnel.	....
Carroll	July 16.	1881	Evening.	NE.	"	....
Morrow and Knox	May 14	1883	5 p. m.	E.	"	900.
Jefferson	June 18.	1883	5 p. m.	NE.	"	450.
Mahoning	March 25.	1884	10 p. m.	NE.	Serpent	165.
Greene	"	1884	4 p. m.	NE.	Hour-glass.	....
Butler	"	1884	4 p. m.	E 25° N.	Funnel.	....
Darke	April 1.	1884	Night.	....	....	....
Madison	"	1884	8:30 p. m.	Easterly.	Funnel.	1,200.
Montgomery	April 27.	1884	5 p. m.	E 5° N.	Hour-glass.	....
Greene	"	1884	5 p. m.	S of E.	Funnel.	200 to 1,320.
Hancock	October 11.	1884	p. m	E.	Inverted cone.	165.
Shelby	June 15.	1885	Afternoon.	NE.	Whirlwind.	65.
Starke	June 21.	1885	7 p. m.	NE.	Funnel.	330 to 495.
Guernsey	"	1885	5 p. m.	NE.	"	200.
Belmont	"	1885	5 p. m.	NE.	"	600.
Trumbull	"	1885	9:30 p m.	NE.	"	60.
Portage	"	1885	7:30 p. m.	N 25° E.	"	500 to 1,000.
Portage	September 8.	1885	10:15 p. m.	NNE.	"	....
Miami and Clarke	"	1885	7 p. m.	E.	"	660 to 2,640.
Fayette	"	1885	7:30 p. m.	ENE.	"	250.
Wyandot and Crawford	"	1885	7 p. m.	NE.	"	1 320.
Putnam	March 20.	1886	8:30 p. m.	NE.	"	200.
Meigs	May 12.	1886	About 10 p. m.	NE.	"	1,320 to 2,640.
Meigs	"	1886	11 p. m.	NE.	....	....
Meigs	"	1886	10 p. m.	ESE.	....	2,640.
Greene	"	1886	10:15 p. m.	NE.	Funnel.	300 to 2,640.
Athens	"	1886	Evening.	NE.	....	600 to 1,200.
Warren	"	1886	Night.	NE.	....	....
Montgomery	"	1886	8 p. m.	NE.	Funnel.	2,640 to 5,280.
Greene and Montgomery	"	1886	Night.	NE.	....	....
Fayette	"	1886	11 p. m.	NE.	Funnel.	2,640.
Vinton	"	1886	10:30 p. m.	NE.	"	1,320.
Fayette	"	1886	10:45 p. m.	NNE.	"	90 to 450.

TABLE II.—*Continued.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Fairfield	May 12.	1886	9:25 p. m.	NE.	"	30 to 375
Mercer	May 14.	1886	Afternoon.	ESE.		
Hardin	"	1886	9 p. m.	NE.	Funnel.	1,640.
Seneca	"	1886	11:30 p. m.	NE.	"	
Hardin	"	1886	11 p. m.	E		
Huron	"	1886	9 p. m.	NE.		
Mercer	"	1886	9:20 p. m.	NE.	Balloon.	1,320 to 2,640
Seneca	"	1886	About 11 p. m.	NE.	Funnel.	500 to 800.
Seneca and Wyandot	"	1886	10 p. m.	NE.		2,640 to 5,280.
Hardin and Wyandot	"	1886	About 11 p. m.	NE.	Balloon.	380 to 2,640.
Seneca	"	1886	11:50 p m.	NE.	Funnel.	250.
Mercer	"	1886	7 p. m.	NE.	"	1,320 to 2,640.
Seneca	"	1886	11:40 p. m.	NE.	"	330 to 550.
Auglaize	"	1886	10 p. m.	NE.	"	2,640 to 5,280.
Allen	"	1886	10:30 p. m.	NE	"	
Gallia	June 13.	1886	6 p. m.	N 10° E.	"	10 to 100.
Fulton	June 24.	1886	2:45 p. m.	ESE	"Bell shaped."	100 to 250
Belmont	July 14.	1886	3:30 a. m.	NE.	Funnel.	100 to 150.
Licking	"	1886	2:30 a. m.?			
Putnam	July 30.	1886	10:40 a. m.	NE.	Funnel.	2,640 to 5,280.
Putnam	August 1.	1886	2:30 p. m.	ESE.	"	1,320.
Delaware	February 10	1887	7:20 a. m.	NE.		Narrow.
Stark	February 11.	1887	7 a. m.	E.	Funnel.	2,640.
Summit	"	1887	7 a. m.	NE.	"	Narrow.
Licking	"	1887	7:20 a. m.	NE.	"	"
Fairfield	"	1887	7:45 a. m.	NE.		2,640 to 3,960.
Knox	"	1887	8 a. m.	E.		Narrow.
Fairfield	"	1887	9 a. m.	NE.		"
Columbiana	"	1887	8:30 a. m.	NE.		"
Athens	March 24.	1887	4 p. m.	NE.		
Gallia	"	1887	5 p. m.	NE.		
Gallia	"	1887	Evening.	NE.		Narrow.
Gallia	"	1887	4:30 p. m.	E		"
Fulton	May 2.	1887	12:35 p. m.	N 20° E.	Funnel.	"
Hancock	"	1887	3:30 p. m.	NE.	Inverted cone	2,640 to 3,960.
Logan	May 30.	1887	12:45 p. m.	N 40° E.	Funnel.	1,320.
Erie	"	1887	2 p. m.	NE.		Narrow.
Seneca	July 8.	1887	1:30 p. m.	E.	Globular.	500 to 1,000.

TABLE II.—*Concluded.*

County.	Month and Day.	Year.	Time.	Direction.	Form of Cloud.	Width of Path in Feet.
Henry	July 21.	1887	Evening.	NE.	Cone shaped.	3,200.
Lucas	September 6.	1887	Afternoon.	NE.	Funnel.	900 to 1,320.
Lucas	"	1887	"	NE.	Inverted cone.	600 to 1,200.
Seneca	"	1887	Night.	Easterly.	....	Narrow.
Hancock	September 7.	1887	3 a. m.	NE.	....	"
Allen	April 5.	1888	Night.	E.	....	1,500 to 1,800.
Wood	"	1888	Evening.	NE.	Funnel.	300 to 500.
Montgomery	May 9.	1888	10 a. m.	NE.	"	100 to 300.
Cuyahoga	May 10.	1888	Afternoon.	NE.	....	Narrow.
Tuscarawas	"	1888	"	NE.	Funnel.	500 to 900.
Stark	May 28.	1888	"	NE.	....	....
Pickaway	"	1888	10:40 a. m.	NE.	Funnel.	600.
Harrison	"	1888	Afternoon.	E.	....	Narrow.
Muskingum	"	1888	Morning.	E	....	"
Lucas	June 13.	1888	Afternoon.	NE.	....	"
Hancock	"	1888	Evening.	E.	....	500 to 1,000.
Seneca	"	1888	"	E.	....	700 to 900.
Richland	"	1888	"	E.	....	1,320.
Belmont	"	1888	"	E.	....	Narrow.
Greene	October 2.	1888	5 p. m.	NE.	....	"

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TABLE III.—*Relative frequency of Tornadoes by months and days, for Ohio.*

The index figures to the right and above the dates show how many times tornadoes occurred on that day of the month.

Month.	Day of Month.	No. of Days.	Total No. of Tornadoes per month.
January	9 and 20	2	2
February	(4)^{2}, 10, (11)^{7}, 13, 14, 18 and 19	7	14
March	4, 20, 22, (24)^{4} and (25)^{3}	5	10
April	(1)^{3}, (5)^{2}, 11 and (27)^{2}	4	8
May	(2)^{2}, 9, (10)^{2}, 11, (12)^{12}, 13, (14)^{16}, 21, 22, 23, (28)^{5} and (30)^{2}	12	45
June	2, (13)^{6}, 14, 15, 16, 18, 19, (21)^{6}, 24 and 28	10	20
July	2, 4, (5)^{2}, 7, 8, (11)^{2}, (14)^{2}, 21 and 30	9	12
August	(1)^{2}, 15, 22, 31 and (—)	5	6
September	(6)^{3}, 7 and (8)^{4}	3	8
October	2, 11 and 20	3	3
November	14	1	1
December	20	1	1
(—) Blank	(—)^{3}	0	3
Total		62	133

NOTE.—The blank (—) signifies date missing.

VERIFICATION OF WEATHER FORECASTS.

By H. Helm Clayton.

The publication in the May Journal of the official rules used by the Signal Service in verifying their weather forecasts is of interest, since heretofore, but few have had access to them, and hence have had no means of determining the value or meaning of the per cents published in the U. S. *Weather Review.* At first thought it seems that if weather forecasts are afterward carefully compared with the observed facts, one can easily determine how many have been succcessful, and how many have failed; and a per cent. of success obtained which, when compared with other forecasts treated in the same manner, will show the relative value of the two. A little consideration, however, will convince one that this is not true. Thus, suppose a weather predictor should confine his forecasts to intervals of a week, and by the side of a column of figures containing the days of the month write "about this time look out for rain." Such predictions might be 100 per cent. verified and yet have comparatively no value. Again, if one confined his forecasts to intervals of a day and merely attempted to foretell whether the day would be fair or foul, he might gain a higher per cent. for forecasts of less value than if he attempted to foretell which part of the day would be fair and which foul. If he attempted to go farther and say whether the sky would be clear, partly cloudy, or cloudy, whether there would be snow or rain, ordinary shower or thunder-storms, and when they would occur, the chances of failure would increase with every added detail, and yet such forecasts which gave, when strictly compared with the facts, only 60 per cent. of success might be of more utility than mere general statements which gave 100 per cent. Few people, however, seem to recognize this fact, and in order that the demand for detailed prediction may be supplied, and yet a fair showing in the way of success be maintained, all the rules for weather verification which the writer has seen, admit of considerable latitude in interpreting phenomena. This it is which frequently

makes the same forecasts, when verified by different persons, vary widely in regard to the per cent. of success. If one is favorably disposed toward certain forecasts, a liberal amount of latitude in verification is generally allowed. The ludicrous extent to which this is carried by a few of the display men in our state weather services who feel personally responsible to their neighbors in having the per cent. of success appear well, and who have no very definite rules for verifying, is shown by instances almost every month of 100 per cent. of success being attributed to the Signal Service forecasts.

In the official rules of the Signal Service, latitude is allowed in several ways:

1st. A rain prediction is considered fully verified when from 60 to 80 per cent. of the district for which the prediction is made is within the rain area.

2d. A prediction of "local rain," etc., is considered 50 per cent. verified whether rain or fair weather prevails over the district, and fully verified if "local rains" occur.

3d. By predicting "clearing weather" the necessity of deciding whether it will rain during the first part of the day is avoided.

4th. Stationary temperature, and higher or lower, are made to overlap each other so that one person might predict "warmer" and another "stationary temperature" for the same day, and yet both be fully verified.

In the official rules used in Germany, latitude is allowed by considering a forecast partially verified even though a phenomenon opposite to the forecast occurs, provided it does not exceed a certain limit in amount. Thus, in the case of rainfall if it does not exceed a certain small amount, the forecast is considered half verified whether the forecast be for fair weather or rain. For reasons stated above, some such latitude seems necessary, but in order that the success of forecasts of different periods may be comparable, the amount of latitude to be allowed ought to be definitely stated so as to eliminate entirely the influence of personal judgment or bias in any direction. In the rules of the German Seewarte, especial pains have

been taken to do this; and the present Signal Service rules seem quite definite, unless latitude is allowed in the estimates of the area of districts covered by phenomena, except in the case of the following rule: "Any prediction of rain will be considered fully verified when from 60 to 80 per cent. of the district is within the rain region during the 24 hours; the percentage allowed will vary according to the size of the district, and the number of stations therein."

Another reason why a certain amount of latitude seems necessary in verifying weather forecasts is on account of the indefinitives of meteorological phenomena. For example, what is considered fair and what foul varies considerably among different persons, and the question not infrequently arises—is a few drops or a light sprinkle of rain sufficient to call a day rainy? or must there be a considerable amount of rain? This is a matter not well defined and about which personal opinions differ, hence the method of the German Seewarte in considering a small amount as an indefinite occurrence to be called half verified, whether fair weather or rain is predicted, does not seem bad. But in order to attach any meaning to a per cent. of success, it is evident that it must be known exactly what is done in such cases. For this and other reasons the definitions attached to the phenomena ought to be made as public as possible. It is not uncommon to find people who have only an indefinite idea of what the Signal Service means by "fair weather."

In order to eliminate these errors and difficulties better methods of verification are generally admitted to be desirable. Dr. Köppen* in Europe, and Professors Gilbert and Doolittle in this country, have made some excellent attempts in this direction, but it is undoubtedly a mathematical problem of much difficulty. A suggestion of some of the things which to the writer seem desirable in a new method of verification, perhaps, would not be out of place. 1st. It is desirable to determine at what point weather forecasts cease to have any value. 2nd. To determine what part of their success can be attributed to chance. 3d. When they deviate from this in the right direction to give

* Meteor Zeitschrift, Oct. 1884.

them a proper weight in the verification in proportion as they approach the exact facts. 4th. To so arrange the material (as is now done by the German Seewarte) that the weak points of the forcasts are pointed out and thus open the road to improvement.

In regard to the first of these it would seem that every one ought to be familiar with the climate of the place in which he lives, and weather forecasts would only become of value as they exceeded the per cent. of success which might be gained by stating well known, or easily ascertained climatic facts.

With the present rules of the Signal Service some approximation to how much the per cent. of success exceeds what might be attributed to chance, or to a mere statement of climatic data, can be obtained by taking the daily forecasts for a certain month, say May 1889, and verifying by the weather of the same month in preceding years. The writer tried this in one instance, and it gave what seemed to him a surprising per cent. of success.

As an illustration of the method of verification which seems to most fully embody the ideas expressed in this paper, an example will be given; and in order that it may not be a purely ideal case, the predictions of weather for Boston and vicinity made at Blue Hill Observatory during the first three months of 1889 will be used. These predictions were made each day, except on Sundays, at 1:30 P. M., and covered the period of 24 hours from the following midnight. The predictions were published in the Boston daily newspapers and purposely so worded as to apply to intervals of half a day. A prediction of "rain" or "fair" was expected to be followed by rain or fair weather respectively during each half of the day. When "rain followed by fair weather" was predicted, rain was expected to occur during the first half of the day and fair weather during the second, etc. For each half day there was thus a separate prediction, and the verifications were made for these intervals. The predictions included the terms "fair," "cloudy," "light rain," "rain," and "snow." Predictions of "heavy rain" are also made at the

Observatory, though none were made during the first three months of 1889. Rules in regard to what should be considered rain and no rain were written out at the beginning of the predictions, but no sharp distinction was drawn between "light rain," "rain," and "heavy rain." Before the verifications were begun, however, the writer who made the predictions, drew up the following as representing as well as could be done in exact figures his definitions of these pheuomena: "Fair weather" = a clear, or partly cloudy sky, with no precipitation, or less than .01 inch; "cloudy weather" = an overcast sky, no bright sunshine, and no precipitation, or less than .01 inch; "light rain" = .01 to .05 inch of rain in 12 hours; "rain" = .06 to .50 inch in 12 hours; "heavy rain" = over .50 inch in 12 hours, and "snow" = over .01 inch (melted) in 12 hours. The predictions were verified by the observations and records of Blue Hill Observatory which includes records from a Campbell-Stokes sunshine recorder, a Pickering pole-star recorder (used for determining the cloudiness at night), and a self-recording rain and snow gauge. It was found that rain and snow fell together in six cases, and on these occasions the phenomenon for purposes of verification was entered under the head of "rain" or "snow," according as the one or other predominated. "Rain and snow" were predicted together on January 16, and February 1. On one occasion rain and snow fell together, and on the other there was light rain. Both predictions were entered under the head of "rain." *

In the following table, illustrating the method of verification, the first letter of each word used in the prediction is entered as a symbol, and will be readily understood. In the column headed PREDICTIONS are entered the number of predictions of each phenomenon; and in the columns headed OCCURRENCES are entered the number of each of the phenomena which followed the prediction. Thus, 99 predictions of "fair" were followed by fair weather 85 times, cloudy weather 6 times, etc.

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* Perhaps in such cases it would be wise to make a separate heading under "rain and snow."

Predictions.	*Occurrences.*					
	F.	C.	L. R.	R.	H. R.	S.
99 F.	85	6	0	3	0	5
17 C.	9	6	0	1	0	1
2 L. R.	1	1	0	0	0	0
33 R.	7	6	4	13	1	2
3 S.	1	0	0	0	0	2
Totals 154	103	19	4	17	1	10

The advantages of such an arrangement, which may be applied to areas as well as to single stations, are decided:

1st. It shows at a glance the directions in which the predictions have been most and least successful. It is seen in this instance that most of the sucesses came from the "fair" weather predictions, while the "cloudy" and "light rain" predictions were oftener followed by other phenomena than by the ones predicted. Attention is thus called to the direction in which improvement may be made.

2d. A collection of such tables will show the relative frequency of each phenomenon and thus prove of much value in climatic studies as well as in guiding future weather forecasting.

Finally, the arrangement is such as to admit of the application of mathematical formulæ for determining the relative amount of skill in forecasting. This will not only enable a comparison to be made between different times and different persons, but will enable one to determine just how much detail can be employed with advantage in forecasting.

By means of the table it is found that 86 per cent. of the Blue Hill "fair" weather predictions were verified; 35 per cent. of the "cloudy" weather predictions; none of the "light rain"; 40 per cent. of the "rain"; and 70 per cent. of the "snow" predictions. If "fair" and "cloudy" weather, that is all the cases in which less than .01 inch of rain was predicted, be included under one head, the number of predictions is 116, and the number verified 106, or 91 per cent. If all of the cases of rain and snow be included under one head, that is all the cases in which more than .01 inch of rain was predicted, the number of predictions is 38 and the number verified 22, or 58 per cent. The

total number of predictions of rain, or no rain, is 154, and the number verified 128, or 83 per cent. The predictions verified at Blue Hill from the same definitions, but in accordance with the Signal Service rules, gave 86 per cent., distributed as follows: January 87, February 89, March 81. The same predictions when verified by Capt. Allen at the Washington Chief Signal Office, from the records of the Boston Signal Service Station, in accordance with the rules of the Signal Service, gave 79.4 per cent., distributed as follows: January 78.5, February 80.9, March 78.9. One of the clerks at the Washington office using the same records, but not instructed in the official rules, obtained 77 per cent., distributed thus: January 67, February 87, March 76. These verifications at the Signal Office were made under instructions from Gen. Greely, and were kindly furnished by him. It is thus seen what differences may result from different methods of verification, and what different results are obtained from the Signal Service rules when interpreted by different persons whose bias, if it exists at all, lies in opposite directions. Mr. Rotch, and the writer compared for January, 1889, the observations made at Blue Hill, and at the Boston Signal Station ten miles to the north, and no appreciable difference in regard to the phenomena used in the verifications was found, so that the differences in the per cent. of verifications must be entirely due to other causes.

But even if no different results were obtained by different persons, the per cent. of predictions verified, as was pointed out before, gives only a poor estimate of the success of the predictions, without a consideration of the number of occurrences which were not predicted. Also, in order to form a true estimate of the value of the predictions, the number which might have been verified by chance coincidence ought to be taken into account.

In the formula for verifying weather predictions worked out by Prof. G. K. Gilbert and published in this JOURNAL, Vol. I, p. 173 (September, 1884), these considerations were taken into account, and this formula can be easily applied to the data in the foregoing table. Thus in the case of "fair" weather the

number of predictions is 99, the number of occurrences is 103, the number of coincidences is 85, and the total number of cases is 154. Then according to the formula the per cent. of skill in inference is 37 per cent. In the same manner the skill in predicting "cloudy" weather was found to be 25 per cent.; "light rain,"—15; "rain," 28; and "snow," 17. Multiplying these per cents. of success by the number of predictions of each and dividing by number of cases the average per cent. of success is found to be 33.

If the predictions as was previously done be divided into predictions of rain or no rain, the amount of skill is found to be 35 per cent., which is almost the same as that given by the average of the detailed predictions, thus showing that the prediction of detail was justifiable.

However, in order to fully express the skill shown in such predictions as these, the writer thinks that Gilbert's formula needs some modification for the following reasons:*

1st. This formula assumes that no skill would be shown in prediction during a given month if fair weather were to be predicted and were to occur every day; but this certainly does not apply to a climate like that of New England, where if a drought of a month should occur it might require the highest degree of skill to predict fair weather every day. In many respects then, it would be better to determine from the normal frequency of any phenomenon the probability of predicting it correctly by fortuitous coincidence, rather than to determine the probability of chance success from the frequency of the phenomenon during the interval covered by the predictions, as done in Gilbert's formula, though this method also has its advantages, since in general it is no doubt easier to predict correctly during settled conditions than variable ones. When, however, the predictions for a long period were verified the two methods would not probably differ.

2d. Gilbert's formula fails to fully express the amount of skill, because in case of predictions involving several degrees of in-

* These strictures apply equally to Doolittle's formula—this JOURNAL, Vol. 2, p. 327, Nov., 1885.

tensity of a phenomenon, it makes no allowance for the extent to which any given degree of intensity is approximated. For example, in the case of rainfall, if "light rain" is predicted and "cloudy" weather or "rain" occurs, it indicates more skill than if "fair" weather occurred. In the case of the Blue Hill "rain" predictions, if the occurrences following the 33 predictions had been distributed in accordance with the frequency of occurrence of each phenomena during the three months, there would have been F. 22, C. 4, L. R. 1, R. 4, H. R. 0, S. 2; but there were F. 7, C. 6, L. R. 4, R. 13, H. R. 1, S. 2, showing divergences of F.-15, C$+2$, L. R.$+3$, R.$+9$, H. R.$+1$, S.± 0. Should not this excess over fortuitous occurrences clustering around R. be allowed some weight in estimating the per cent. of skill in predicting R.? and if so, how much? The writer tried a number of solutions for this problem, but found none satisfactory to himself.

Blue Hill Observatory, July, 11, 1889.

DISTRIBUTION OF AVERAGE WIND VELOCITIES IN THE UNITED STATES.

Investigated by Dr. Frank Waldo.

PREFACE.

In selecting the subject of Mean Wind Velocities as one for study, the idea of practical utility as well as scientific interest was in mind. In the present paper a variety of points in connection with the amount of wind are treated, but none of them have been fully developed, as it was necessary to keep this paper within narrow limits; and in a first work of this kind it seemed better to touch on several of the associated topics rather than to devote the whole paper to the more or less complete discussion of any one of the subdivisions to be found in the paper.

While little practical use has been found for data such as is about to be considered, yet it will undoubtedly find application in the near future.

Take for instance, the subject of Fire Insurance. The risk

of loss by fire must be very much greater where strong winds occur than where light winds do. The cities of the sea-coasts having more wind than inland cities are more liable to destructive fires: a town located on the Great Plains, as for instance Dodge City, is more liable to destruction by fire than a Southern City, say Augusta, Georgia. So, too, the seasons of strongest wind are the most dangerous for any given region; and in the same locality houses or towns set up on hills are much more of a risk than those in valleys. It is evidently possible for insurance men to take into account the winds for various localities, not only in the whole country, but even for small districts in the same city, if they know the wind distribution of the country and the law of increase of wind with elevation.

The use of wind-mills can be greatly increased if it is shown that a certain amount of wind can be counted on for each month in any particular region. Also it can be foretold at just what height the wheel must be exposed in order to obtain a given power, if we know the amount of wind for any plane and the law of increase. For instance, on the western plains a windmill would raise nearly three times as much water, for irrigating purposes, as could be raised in portions of the south, for the same moderate elevation above the earth.

For engineering purposes, too, the average wind data is very useful, but not so important as the force of wind gusts.

It seems as though the development of Dynamical Meteorology demands a thorough study of wind velocities in all its phases, and especially in the vertical distribution. It is hoped that the present chapter on this latter subject will be of use in theoretical studies, especially the portion treating of the annual march of increase of wind velocity with elevation. Professor Ferrel's theoretical results on the subject of wind velocities at various elevations would be of much more value if he could obtain "constants" from combinations of observed wind data at the elevated stations on Mt. Washington, Pike's Peak, Mt. Hamilton, Blue Hill and Cape Mendocino in our own country, and such stations as Victoria Peak, Sandwich Island Peaks, Ben Nevis, Puy de Dome, Pic de Midi, Santio, Sonnblick, etc., of

foreign countries, together with the surface wind data near these places. There has already been accumulated an immense amount of this data, but much of it is not in form for discussion and application.

The discussion of the average wind velocities for a number of years, appears also to furnish about the only reliable method of determining friction relations over land and water; and the continental distribution of the winds are necessary for looking at this question from all sides.

A portion of this paper was published in the Meteorologische Zeitschrift last Fall, but I did not know until the present manuscript was being copied that any one else was studying a similar question. On March 12, 1889, a copy was received of a paper by J. Kiersnowsky, titled "Uber den Täglichen und Jährlichen Gang und die Vertheilung der Windgeschwindigkeiten im Russischen Reiche." This paper was presented to the Russian Imperial Academy of Sciences on October 4, 1888, and is printed as No. 3, Band XII of Wild's Repertorium für Meteorologie, St. Petersburg, 1889. Just before his paper was presented to the Academy the author received my preliminary paper, concerning which he gives the following postscript to his own work. "Erst nach Beendigung meiner Arbeit sowie nach der Construction der Isodynamen des Windes kam mir die verwandte Arbeit von Herrn F. Waldo: 'Mittlere Windgeschwindigkeit in den Vereinigten Staaten' (Meteorologische Zeitschrift, V Jahrgang, 1888, Heft 8, August), zu. Herrn Waldo stand zu seiner Untersuchung und zur Construction der 'Isodynamen' ein bedentend genaueres Material (Signal Service obs.) zur Verfügung als mir, da die Windgeschwindigkeit auf den Stationen der Vereinigten Staaten nach den Angaben Robinson's cher Anemometer bestimmt wird. Gleich wohl zeigen die Resultate des Herrn Waldo, insoweit sie überhaupt vergleichbar sind, eine befriedigende Uebereinstimmung mit den meinigen."

Section I. Introduction.

Many papers on the direction of the wind have been published, and the wind distribution is pretty well known for most

navigable and inhabitable portions of the globe for the wind direction, but there is still much to be learned in regard to wind velocities.

Concerning the wind velocities, the U. S. Signal Service has accumulated a mass of data of the utmost value, embracing as it does a continuous registration of the velocity of the wind (every mile) for all of the principal stations from the time of their establishment up to the present time, or to the time when the stations were discontinued. This is without doubt one of the most valuable of the scientific works accomplished by the Signal Service, because the wind is the only element recorded by self registering instruments at the Signal Service Stations (except the maximum and minimum temperatures.)

The observations are made by means of Robinson anemometers of good construction, which have an electrical attachment by means of which a circuit is closed for every mile of wind, and causing a record to be made on the recording sheet of a small cylinder chronograph. These chronographs are of cheap and somewhat inferior construction, but are probably accurate enough for the purpose for which they are designed.

The anemometers are mounted on the roof of the Signal Service building at Washington and compared with a standard anemometer before they are sent to other observing stations. If the readings of the new instrument differ by more than four per cent. from the standard, it is rejected, otherwise it is considered sufficiently accurate. These comparisons must usually take place at quite low velocities of wind, and consequently the test is not sufficient for anemometers for stations where the high winds occur. No subsequent comparison of the instrument with a standard is made after it is mounted at the station where it is to be used, and it is supposed to remain in perfect condition unless it needs some repairs, when it is returned to Washington for such repairs to be made, in which case it is again compared with the standard before being sent to an observing station again. About three years ago, the question of Inspectors carrying a standard anemometer as well as barometer and thermometers was urged on the Signal Service, but with what result I do not

know. Anemometers certainly need comparison with a standard from time to time, as the friction constant undoubtedly changes, but such comparisons have not been made in the series of obervations we are about to discuss.

The instruments are supposed to be all of the same dimensions, but this is not absolutely so, as we shall see later.

At Mt. Washington (and probably at other exposed stations) anemometers of unusually strong construction are used, in which the arms are braced, and I think the electric contact is so arranged as to close the circuit for every four or five miles instead of for every mile as in the anemometers for other stations.

In the "Instructions to Observers" printed in 1881, there is given diagrams of the wind measuring apparatus and the self-registers for both direction and velocity. In a pamphlet titled "History of the Signal Service," Washington, 1884, there is also a diagram of the anemometer and the registering apparatus for the same; and in this official pamphlet, page 17, is the following remark: "Experiments have shown that the velocity of the cups in all cases is approximately one-third of that with which the wind blows, no matter from what point of the compass it comes; and that the relation between the velocity of the cups and that of the winds, is independent of the size of the instrument. As the distance traveled by the wind is three times that travelled by the cups, the velocity of the former can be easily deduced. Generally it (the anemometer) is placed twenty feet above the roof of the building in which the office is located."*

These and more recent remarks show that the Robinson factor 3 is still adhered to by the Signal Service, and in fact all of the tables of the wind velocities published by the Signal Service and given in miles per hour are computed by means of this still universally used but erroneous factor. It is unfortunate that with the introduction of the Robinson instrument, this erroneous reduction factor came into use, which it has, at least so far, been impossible to get rid of and which gives very mis-

* Since the above was written the Signal Service has published some results of investigations of anemometers, but have made no change in the "reductions factor" used in obtaining the published wind velocities.

leading results for high wind velocities. The motion of the center of each of the four hemispherical cups around the axis of the anemometer is not to be multiplied by the factor 3 in all cases in order to obtain the "wind way" causing this motion. This number universally known as the Robinson factor has been, and is, applied to cup anemometers of all dimensions irrespective of the size of the cups and length of arms supporting the cups. For some fifteen years now, it has been known that this factor varies not only for different relative dimensions of the anemometers, but also somewhat for different wind velocities for the same anemometer.

The Combe's Whirling Apparatus seems to be gradually becoming the standard for testing anemometers, and unless some new and valid objections are offered against its use it will probably be adopted as the standard for all meteorological services. For even if results obtained by it are found to be somewhat incorrect as regards absolute measures, yet its convenience and uniformity of results would still recommend it, and the *absolute* correction could be easily determined if some one would spend the necessary time in experimenting. This apparatus has been described in so many places that I will only state here that it consists essentially of a horizontal arm whirled around a vertical axis rigidly attached to one end, the anemometer being mounted on the upper side of the free end in such a way that the cups will be horizontal.

The anemometer passes over a certain space in a certain time, during which the cups are made to revolve by the resistance offered by the air. The ratio of the space passed over to the amount of wind as shown by the revolution of the cups (the anemometer constant) Robinson found to be represented by a single term; Dohrandt, however, from an investigation of anemometers of various sizes, deduced the frequently quoted formula

$$W = K + A\left(3.0133 - 53.7367\frac{R^2}{r} + 1033.81\frac{R^4}{r^2}\right.$$

where *W* is the true wind velocity (space passed over by the anemometer), *K* an instrumental constant, sometimes called the

friction constant,* A the velocity of the anemometer cups, R the radius of the anemometer cups, and r the distance from the centre of the anemometer spindle to the centre of the cups.

Köppen has found that K can in general be taken as 1 meter per second (2.2 miles per hour), but this is probably a little too high.

Taking for the Signal Service anemometers the distance of the centre of the cup from the axis of rotation at 170.7 mm., and the radius of the cup as 50.8 mm., I deduced the following formula for the reduction of anemometer velocities to true velocities:

$W = 1$ (m. p. s.) $+ A \times 2.437$ by means of Dohrandt's formula and Köppens friction constant.

Director Neumayer, of the Deutsche Seewarte at Hamburg, tested two †Signal Service anemometers directly on the Seewarte Combes apparatus, in which he found for

Anemometer	No. 34	$w = 0.751 + 2.608 \times n = 0.75$ m p. s. $+ 2.405 \times a$
"	No. 449	$w = 0.888 + 2.628 \times n = 0.89$ m. p. s. $+ 2.450 \times a$

where n is the number of revolutions of the anemometer cups, and a the distance passed over by the cups.

It must be remarked, however, that the anemometer No. 34 had a slightly longer arm than No. 449. The dimensions were, for No. 34, 172.6 mm., and for No. 449, 170.7 mm. I have the impression that the anemometer No. 34 is made of greater strength than the other, and is such as is used at stations having unusually strong winds, such as Mt. Washington.

It is seen that the friction constant, .89 m. p. s., differs by only .11 m. p. s. from that adopted by Köppen, and the coefficient of a, as observed at Hamburg, differs by only 1 % of its amount from that given by Dohrandt's formula, obtained by means of a totally different Combes apparatus at St. Petersburg. I wish to call particular attention to the agreement of these results as

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* I quite agree with Prof. Köppen on his use of the term "friction constant" so long as the simple linear equation $W = K + A\,R$ is used.

† Prof. Marion has published his results since the above was written; he finds for Sig. Serv. Anemometers

(1) $w = 0.225$ (m. p. h.) $+ 3.143\,a - 0.362\,a^2$ or, (2) $w = 0.994$ (m. p. h.) $+ 2.739\,a$

tending to show the uniformity of the results obtained by means of the Combes apparatus under different conditions.

I have personally examined the mountings and surroundings of these two instruments for comparing anemometers and have seen them in operation (was present during a portion of the testing of one of the above mentioned Signal Service anemometers) and find that the conditions of *exposure* are very unlike; and so much so, that an agreement of the results from the two instruments must strongly urge the accuracy of the results obtained with either of them.

Very lately there has been published by Dubinsky a direct comparison of the two Combes apparatus by means of two small anemometers tested first at Hamburg and then at St. Petersburg.

The lengths of the whirling arms are Hamburg = 3.843 meters; St. Petersburg = 3.3158 meters.

The anemometers were small, such as might well be used by travelers or at sea.

Their dimensions were,	Anemom. No. 74	Anemom. No. 75.
Radius of cup	R = 0.020175	R = 0.020125 meters.
Distance of cups centre from axis of rotation	r = 0.049900	r = 0.05078 meters.

The velocities at Hamburg ranged from 4 to 60 kilometers per hour, and at St. Petersburg from 10 to 77 k. p. h.

The final results obtained by Dubinsky are:

ST. PETERSBURG.

Anemom. 74. $V = 0.961 + 0.92032\,c - 0.000256\,c^2$

$V = 1.262 + 0.899\,c$

Anemom. 75. $V = 0.843 + 0.95127\,c - 0.000704\,c^2$

$V = 1.367 + 0.9054\,c$

HAMBURG.

Anemom. 74. $V = 0.759 + 0.94028\,c - 0.000492\,c^2$

$V = 1.027 + 0.9166\,c$

Anemom. 75. $V = 0.650 + 0.97846\,c - 0.000961\,c^2$

$V = 1.195 + 0.9194\,c$

In these formulæ V is the velocity of the anemometer in kilometers per hour, the first term in the second number is in

kilometers per hour, and c is the number of the contacts made in an hour by the electric apparatus in connection with the cups, the mean being taken for the motion of the whirling arm through N.W. S.E. and N.E. S.W. (a contact being made for each 1,000 revolutions of the cups). It is considered that the first equation in which c^2 is introduced as a third term is a little more accurate for high velocities, but usually the second equation is considered sufficient. It is seen that there is very little difference between the results obtained by means of these two Combes' whirling machines. I have entered so minutely into the results obtained at Hamburg and St. Petersburg because it is necessary to know the relation between the *miles of wind* as indicated by an anemometer and the true wind velocity as obtained by linear measure. So little has been done in the way of working up observations of wind velocity (except for the daily period) that the inaccuracies at present existing have not been seriously felt, and practical meteorologists have not made any combined action toward rectifying the error into which all have been led. Each one has followed the old method, because so many observations have been published using the Robinson "factor," that the old and new observations would not be comparable with each other if a new "factor" was adopted. For this same reason I have adhered to the factor 3 in all cases except those specially designated in the text. In most cases where the wind velocities have been investigated it has been purely relative values that have been discussed, and so the use of erroneous wind miles has not made so much difference; but even in these cases, where the observations made in different observatories using anemometers of various dimensions are compared, considerable error may be introduced.

The greatest error arises, however, when we wish to compare wind velocities with absolute motions. Take for instance the comparison of amount of wind with the progress of areas of high or low barometric pressure from one geographical location to another. If we use the present anemometer mile in this case it might lead us to very erroneous conclusions by our finding seeming relations which do not actually exist. Also, if we

wish to determine some law of increase of velocity with the elevation above the earth's surface, this law would be incorrect for absolute motions such as are obtained by means of cloud or balloon observations; and the law obtained by means of a pressure anemometer would not be the same as for a velocity anemometer. And, too, in the determination of wind gradients the large wind velocities sometimes observed are very misleading. As an extreme case take the Mt. Washington velocities of 150 miles an hour. These reduce to only about 124 miles per hour when we use the true English miles. These are only a few of the cases which might be cited where it is absolutely necessary to use true wind miles and not anemometer miles.

[TO BE CONTINUED.]

CURRENT NOTES.

POLARIZATION OF THE LIGHT OF THE SKY.—Herr Gymnasiallehrer Busch, of Arnsberg, has published in the *Met. Zeitschrift* for March, an interesting study of the polarization of the light of the sky. In 1886 he had found a heretofore unknown regularity in the movements of the two neutral points (Arago's and Babinet's). In the present paper he shows that the negative polarization was notably increased during the optical atmospheric disturbance of late years, and that Babinet's neutral point was always to be found in the brightest part of the purple light of the twilight colors. He also reached some novel conclusions as to the planes of polarization, which do not admit of easy summing up.

INTERNATIONAL METEOROLOGICAL TABLES.—The International Meteorological committee having completed the new Reduction Tables which have been for some time in preparation, the sub-committee consisting of Mascart and Wild, having charge of the details of selection, arrangement and editing, has issued a circular dated July 20, 1888, calling attention to this very important publication. There has, for

some years, been an increasing necessity for a collection of Tables, such as could be recommended for adoption by all countries. Almost all of the extensive meteorological services have tables of their own and these do not always agree with each other in details. The tables most extensively used at present are the Smithsonian Tables by Guyot—we have found these in practical use (where extended tables were necessary) in nearly all of the great European observatories; but even the newer edition does not fill the wants of the present meteorological workers. So it is quite probable that these International Tables will become to the newer generation of meteorologists what Guyot's were to the older. Coming as they do with the sanction of the highest meteorological authority, that of the International committee, any objections which individual meteorologists may have in regard to their completeness or utility, should not stand in the way of their universal adoption.

The above mentioned circular with specimen pages and table of contents was received some time ago, and this occasion is taken for bringing the contents to the notice of the readers of this journal. The following table shows the scope of the work:

First Part. Text. Description and use of Tables (in three languages, French, German and English).

Second Part. Tables. (with French, German and English headings).

Chapter I. Units of measure—Comparative values of the units. Section I, measures of length. Section II, measures of weight. Section III, measures of time, and angular measure.

Chapter II. Geodetic measures.

Chapter III. Thermometers. I. Conversions of different scales. II. Reduction of Temperature to the Sea Level.

Chapter IV. Barometers. Conversion of Scales.. Reduction to 0°C. Reduction to Sea Level (with Logarithms).

Chapter V. Hygrometry. Rain. Evaporation.

Chapter VI. Wind.

Chapter VII. Magnetism and Electricity.

The total number of pages is 420, quarto, and the specimen press work is as beautiful as good paper, clear type and broad

margin can make it. But the name of Gauthier-Villars et Fils. as publishers is sufficient guarantee for a good appearance. The price of these tables, 35 francs (almost $10.00 when the import duty is paid), will prevent most individuals from buying a copy, but all of the officially published results of general government, state and private observations should be reduced as far as possible by means of these new International Tables. (Prof. E. Mascart—176 Rue de l'Université, Paris, will receive communications concerning the tables, and they can be readily procured through him). FRANK WALDO.

UNITED STATES.*—Reprints of special articles from the *Encyclopedia Britannica* have occasionally formed volumes of considerable scientific value—Herschel's "Meteorology," for example, being one of them—and Whitney's "United States" being a recent example from the latest Ninth Edition. The volume is more than a reprint, for the preface tells us that it presents the article as written for the Encyclopedia, and not as it was cut and condensed by the editors to reduce it to the allotted space. The contents include chapters on the physical geography and geology of our country, its political and natural subdivisions, its climate, its forests; a chapter called scenographical population and immigration, the public lands, a detailed statement of our mineral resources and agriculture, manufactures and foreign commerce. An appendix contains a sketch of geographical exploration in the West, of the progress of our cartography, and on the methods and probable accuracy of the measures of altitude of our mountains.

The chapter on climate is most pertinent to our reading. We find here an account of the general distribution of temperature and the annual variations, in accordance with which the country is divided into an Eastern region, containing three-fifths of our area and nineteen-twentieths of our population; a plateau and mountain cordilleran region farther west, sparsely populated, and a Pacific Coast region, smaller than either. Schott's charts are the chief authority for this section. The winds of the

*The United States. J. D. Whitney. Little, Brown & Co., Boston, 1889; large 8vo. 472 p.

country are taken for the most part from Woeikof's discussion of Coffin's data. Special mention is made of the summer day winds of the Golden Gate, blowing inland strongest in the hottest weather, towards the mountains; but at night, there is a calm at San Francisco, while the cooled air flows in a gentle breeze down the mountain slopes. Schott is again the chief authority quoted on precipitation; twenty inches annual rainfall is regarded as the minimum for cultivation without irrigation. Cold and hot waves are described, but are placed before the account of our cyclonic storms. Lewis Evans is quoted as the first to make a generalization of value in reference to the storms of the United States, but it is questionable if this should not be credited to Franklin. Loomis is followed in the account of our cyclonic storms, and Finley for the tornadoes; the omission of thunder-squalls and of the chinook wind is to be regretted. The latter is highly characteristic of the plains close to the Rocky Mountain front, and the former are probably to be credited with the "thirty minutes" duration of so-called tornadoes in Finley's list. The tornadoes of February 19, 1884, are mentioned as the most remarkable occurrence of the kind on record, and the cyclonic snow storm of March 11–14, 1888, is deservedly placed at the head of its class, the data in regard to it coming chiefly from Upton's account in this JOURNAL.

The work is essentially a statement, not a discussion, and forms a most valuable and carefully prepared collection of "facts and figures" illustrating the physical geography of the country and its physical resources. W. M. D.

METEOROLOGICAL APPARATUS AND METHODS.* — We regret that Professor Abbe's treatise should have been published as an appendix to the Chief Signal Officer's Report, for even though it occupies a volume by itself, it will not in this form attract the general attention which an independent work by Professor Abbe would naturally receive. By reason of his

*Treatise on Meteorological Apparatus and Methods. by Cleveland Abbe, A. M., Professor and Assistant in the office of the Chief Signal Officer Appendix 46 and Part 2 of the Annual Report of the Chief Signal Officer for 1887. Octavo, 388 pages, with plates.

extensive acquaintance with the meteorological literature of this country and of Europe, no one is better qualified than Professor Abbe to handle the subject, and he has produced the only work of the kind existing, which will be invaluable to all students of instrumental meteorology who have hitherto been confined to general treatises on meteorology and to the files of magazines for descriptions and discussions of apparatus and methods.

We have noted in a few cases that certain meteorological apparatus of American design has not been mentioned, while the corresponding European apparatus has been described, but this may result from the greater opportunities afforded abroad of making known the merits and defects of new instruments, through the medium of the journals devoted to meteorology in Europe. It may also be said that the concluding chapter on the measurement of rain and snow is too brief for such an important subject, and that it appears to have been curtailed to complete the volume. The extensive subjects of optics, electricity, and actinometry remain to be described in a subsequent volume.

Professor Abbe says in his preface: "Since 1653, when the thermometer and barometer had become available, new instruments or improvements on the old have been invented, and the present treatise is intended to set forth both the progress and the present condition of our knowledge of the methods of accurately observing the fundamental data of meteorology." In the arrangement of the chapters there is first given a general description of the object to be attained, second, a development of the formulæ for correcting the errors of the apparatus, and finally, an indication of the refined methods of making standard determinations, to which all ordinary practical methods are to be considered as approximations. One of the most serious obstacles to exact observations has been the great diversity of standards and instruments disseminated among observers. In this respect the past decade has seen great improvement, especially through the recognition of the International Bureau of Weights and Measures as the ultimate authority in respect to

all standards. The author acknowledges the services rendered by Professors Waldo and Curtis, formerly of the scientific staff of the Signal Service, in the preparation of his work. The titles of the sections and chapters are as follows:

The Measurement of Temperature.— General History of Thermometers. Modern Thermometry; the Air Thermometer. The Mercurial Thermometer. Exposure of Thermometers. Miscellaneous Forms of Thermometers. Thermographs.

The Measurement of Atmospheric Pressure.—The Barometer in General. The Corrections of the Normal Mercurial Barometer; International Comparisons. Miscellaneous Barometers. Barographs.

The Measurement of the Motion of the Air.— General Remarks. Measurement of the Direction of the Wind. Direct Measures of Wind Velocity. Personal Estimates and Arbitrary Scales of Wind Force. Pressure Anemometers. Suction Anemometers. Rotation Anemometers. The Movements of the Upper Currents.

The Measurement of Aqueous Vapor.

The Measurement of Precipitation.

At the end of the volume are plates illustrating the apparatus previously described, to which there is an index. It will be seen from the above that Professor Abbe deals with the principles as well as with the details of construction, so that future inventions or modifications can be relegated to their proper place in the classification. While we are surprised at the variety of instruments of precision which have been enlisted in the service of meteorologists, it is certain that the number will rapidly increase. A. L. R.

International Congress of Hydrology and Climatology. —In accordance with the decision of the Congress at Biarritz, in 1886, the second session of the International Hydrological and Climatological Congress will be held at Paris Oct. 3-10 of this year. Scientific societies and associations and French and foreign scientists are invited to take part at this International reunion. The committee calls attention to the question in the

following programme, most of which will form the subject of a statement or report which will be published and sent to subscribers three months before the opening of the congress, and will serve as the basis of discussions at the meetings. Besides the questions on the programme, other memoirs on hydrology and climatology may be submitted. The proceedings of the Congress will be published by a special committee and sent gratis to members. The committee hopes that all who are interested in the study of hydrology and climatology will take part in the seances of these two branches of medical science. The president of the committee is M. E. Renou, director of the meteorological observatory of the Park of St. Maur, and the general secretary is Dr. F. de Ranse, Néris Allier, France. The membership fee which is twenty francs, should be sent to the treasurer, M. O. Doin, publisher, 8 Place de l'Odeon, Paris.

The following is the programme of questions proposed by the committee:

HYDROLOGY.

(a) Scientific hydrology. Precautions to be taken to determine the precise temperature of thermal springs.

Relations between mineral waters and geological strata. Micro-organism contained in mineral waters and their influence on the composition and properties of these waters.

Influence of the doctrine of microbes on thermal therapeutics.

Origin of the gas contained in mineral waters and the part which it plays in the properties of these waters.

Vapors which are emitted from mineral waters and their transformations.

Programme for a course of instruction in hydrology.

(b) Medical hydrology. Treatment of various diseases.

CLIMATOLOGY.

Conditions which should obtain in the installation of a meteorological observatory for medical purposes.

Organization of weather predictions at sanitary stations. Rules for weather forecasts.

Climatology of various sanitary resorts.

Comparison and classification of sanitary stations with respect to their climatological conditions.

Effect of high altitudes on chest diseases.

Effect of maritime climates on tuburcular diseases.

Programme for a course of instruction in climatology.

EVANS AND FRANKLIN.*—Another suggestion from Franklin was that our northeast storms came from the southwest; and with this began the science of weather prediction. Franklin's first mention of this was in a letter to Jared Eliot, dated at Philadelphia, July 16th, 1747. He wrote: "We have frequently along the North American coast storms from the northeast, which blow violently sometimes three or four days. Of these I have had a very singular opinion for some years, viz.: that, though the course of the wind is from northeast to southwest, yet the course of the storm is from southwest to northeast; the air is in violent motion in Virginia before it moves in Connecticut, and in Connecticut before it moves at Cape Sable, etc. My reason for this opinion (if the like have not occurred to you) I will give in my next" (Sparks' *Life of Franklin*, vi, 79).

The reasons are stated as follows in a letter to the same correspondent, dated Philadelphia, February 13th, 1749–50: "You desire to know my thoughts about the northeast storms beginning to leeward. Some years since, there was an eclipse of the moon at nine o'clock in the evening, which I intended to observe; but before night a storm blew up at northeast, and continued violent all night and all the next day; the sky thick-clouded, dark and rainy, so that neither moon nor stars could be seen. The storm did great damage all along the coast, for we had accounts of it in the newspapers from Boston, Newport, New York, Maryland and Virginia; but what surprised me was, to find in the Boston newspapers an account of the observation of that eclipse made there; for I thought as the storm came from

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* From a lecture by Professor W. M. Davis before the Franklin Institute.

the northeast, it must have begun sooner at Boston than with us, and consequently have prevented such an observation. I wrote to my brother about it, and he informed me that the eclipse was over there an hour before the storm began. Since which I have made inquiries from time to time of travellers, and observed the accounts in the newspapers from New England, New York, Maryland, Virginia and South Carolina: and I find it to be a constant fact that northeast storms begin to leeward, and are often more violent there than to windward. Thus, the last October storm, which was with you on the 8th, began on the 7th in Virginia and North Carolina, and was most violent there." (*Id.*, vi, 105, 106).

Some writers quote Lewis Evans as the first person to recognize the general movement of our storms. Thus in *Weather Charts and Storm Warnings* (London, 1876, p. 80), Scott says: "The earliest notice of it [the translation of storms] which we can discover is an entry on a map of Virginia, published in 1747 by Lewis Evans, to the effect that all our great storms begin to leeward. Franklin, in 1760, followed in the same strain, but it appears that his attention had been caught at an earlier period, in 1743." Evans was a well known geographer of the middle of the last century, and his "Map of Pensilvania, New Jersey, New York, and the three Delaware Counties" (1749) was highly esteemed. Following the fashion of those early days, certain parts of the map, of whose topography the author had little information, were occupied with legends explanatory of one matter or another; and among these statements is the following: "All our great Storms begin to Leeward: thus a NE Storm shall be a Day sooner in Virginia than in Boston. There are generally remarkable changes in the Degree of Heat and Cold at Philadelphia every 3 or 5 Days, but not so often to the Northward. * * * Thunder never happens, but with Meeting of Land and Sea Clouds. The Sea Clouds coming freighted with Electricity, and meeting others less so, the Equilibrium is restored by snaps of Lightning; and the more opposite the Winds, and the larger and compacter the Clouds, the more dreadful are the shocks. The Sea Clouds, thus suddenly bereft

of that universal Element of Repellancy, contract, and their Water rushes down in Torrents."

It seems to me very likely if not certain that Evans learned all this from Franklin, and that he wrote it on his map just as he did any other pertinent information that he could gather. The two men were both residents of Philadelphia, and Franklin's letters make such mention of Evans that we may be sure that they knew each other well. Franklin and Hare were publishers in 1755 of a little pamphlet of text that went with a later edition of Evans' map, on which, by the way, the statement about storms quoted above was crowded out by new geographic matter. Evans' statements about thunder-storms bear the clearest witness to Franklin's influence. Writing to Collinson in 1749, Franklin said: "For, if an electrified cloud, coming from the sea, meets in the air a cloud raised from the land, and therefore not electrified, the first will flash its fire into the latter, and thereby both clouds shall be made suddenly to deposit water." (*l. c.*, v., 216).

We can hardly suppose that Evans, who nowhere but on the first edition of his map makes mention of the identity of lightning and electricity, was an original investigator in this subject; and as he evidently used Franklin's ideas in this matter, it is likely that he did the same with the northeast storms. I do not say this with any idea of discrediting Evans; he was an able man, as appears from his excellent geographic descriptions, which show an admirable perception of the physical features of our country.

Moisture Economy in Kansas.—In the Sixth Biennial Report of the State Board of Agriculture of Kansas, we find that the question of economy of moisture in that State is receiving some attention. Two of the four papers relate to this subject. Mr. E. R. Hilton views the subject from the standpoint of the condition of the surface soil and subsoil. The following are paragraphs taken from his paper:

One theory of general acceptance is that the loosening of the soil more freely admits the air, which gives up part of its mois-

ture when brought in contact with the cooler soil. This theory will be true while the soil is cooler than the atmosphere, but when the atmosphere is cooler than the soil, which will generally be the case between midnight and morning, then the soil must, on the same principle, give up part of its moisture to the atmosphere. In striking a balance the greater gain would probably be with the soil, but the actual gain of the moisture obtained from this source will not account for the benefits that frequent loosening of the soil confers when showers are infrequent and temperature high, and we must attribute the good results that follow, not to the moisture robbed from the atmosphere, but rather to that saved and economized in the earth.

Of the rainfall over a large area of the country it is estimated that less than one-half finds it way back to the sea, more than one-half being evaporated back into the atmosphere again. No tests have been made in Kansas to determine what percentage of the rainfall is drained off in our streams, but owing to the larger acreage of unbroken primitive sod than of cultivated land, the surface drainage will naturally be greater than in countries where the greater part of the soil not covered by native timber has been cultivated at some time in the past. Eastern Kansas, owing to larger relative acreage cultivated, probably drains off less than half its rainfall, while western Kansas drains more than half. The water drained from a deeply-cultivated field will, however, be much less than the average of the country and the amount evaporated from it much greater. Only in instances when the rainfall is excessive within a given time will there be much waste by surface drainage from a well-cultivated field. What escapes is mainly by sub-drainage. We may say, therefore, that the rainfall drained out of a country is, so far as the growing crop is concerned, rainfall wasted or lost, and the rainfall that is held in the soil and evaporated back into the air again is rainfall utilized or saved.

Two important questions are before the trans-Missouri farmer of to-day: one, how to increase the percentage of rainfall saved and utilized, and the other, how to increase production on a given rainfall, or in other words, how he can work his farm so

as to make an inch of rainfall last longer, and give better returns than in the past; make two blades of grass grow where one grew before, by making one inch of rain do what two did before. The thrifty farmer practices economy in every branch of his work; the successful farmer must also practice strict economy of every drop of rain that drains into the soil, to get the best results from his farm.

The water in the soil, not drained into the streams, is evaporated at the surface or transpired by the growing plants. By the law of capillary attraction water ascends through the spaces between the fine particles of sand or clay from the reservoirs below. The finer the sand or particles of clay, the smaller will be the spaces between, and the higher water will rise through these small nature-made capillary tubes. In very porous or friable clays or coarse sand or gravel the height that water will rise by capillary attraction is limited. The quantity of water raised in a given time depends on the capillary power of the soil. If fine, and capable of raising water twenty to twenty-five feet, the process will be slow. If capable of raising water ten feet, it will be much more rapid, the quantity being gradually reduced as the height is increased.

The same law applies to the drainage of the water from the surface and storing it after a fall of rain. The surface soil to depth cultivated takes in the water rapidly until fully saturated. If rain continues falling faster than the subsoil will absorb it, then the surplus must waste at the surface. The amount stored for future use is determined by the nature of the subsoil and capacity to absorb. If the capillary spaces are small, drainage will be slow; if large or coarse, it will be rapid. The law that elevates the moisture to take the place of that evaporated at the surface also draws or sucks the water down from the surface to the water reservoirs, or water level below, after each rain. It follows the law of supply and demand, ascending or descending, as the point of greatest deficiency is at the surface or at the water level.

The high temperature rapidly dries out the surface soil, and would rapidly exhaust what was stored in the substrata, did we

not take prompt measures to protect our reservoirs by cultivation. The moisture rises just as far as the soil is compacted, and finds its upper limit at the roots of the plants where it is needed. The mulch of two or three inches of loose soil forms a cool covering to protect it from the evaporating heat at the surface. The moisture in the subsoil is thus saved and economized for future use.

I feel confident that the experience of many farmers will confirm the statement that on clean ground where the suppression of weeds does not figure, the corn that has been cultivated as soon after every packing rain as practicable, has given the best results.

As we study this law of capillary attraction, and understand the important relation it bears to successful farming, we must be impressed with the importance of a better knowledge of the physical properties, not only of the surface soil we cultivate, but also of the sub-strata. The method of cultivation will depend on the nature of the surface and the subsoil.

While the question of preserving the fertility of the soil is now, and always will be, an important one, at present the question of preserving and economizing moisture to keep the plant in healthy growth until matured, is still more important. If one of the two is to be sacrificed, it will be the latter.

Our farmers have been and are now handicapped by lack of proper legislation. In order to reach the best results and do their work intelligently, a geological survey of the State is needed, which, in addition to its work in ascertaining the mineral resources of our State, shall also classify our different kinds of soil and sub-strata within 25 to 40 feet of the surface. Our farmers need also that the work of our Agricultural College be extended and enlarged so as to establish experimental stations on different kinds of soil in different parts of the State; the sum of the information thus obtained to be compiled at the College and given to the public from time to time through the medium of monthly, quarterly or biennial reports of the State Board of Agriculture.

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